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RESEARCH AND THE FUTURE OF ENGINEERING PSYCHOLOGY

Jack A. Adams

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DOCUMENT CONTROL DATA - R & D

(Security classification of title, body of abstract and indexing annotation must be entered when the overall report is classified)

1. ORIGINATING ACTIVITY *(Corporate author)*	2a. REPORT SECURITY CLASSIFICATION
Aviation Research Laboratory	Unclassified
Institute of Aviation	2b. GROUP
University of Illinois, Urbana, Illinois	

3. REPORT TITLE

RESEARCH AND THE FUTURE OF ENGINEERING PSYCHOLOGY

4. DESCRIPTIVE NOTES *(Type of report and inclusive dates)*

Scientific   Interim

5. AUTHOR(S) *(First name, middle initial, last name)*

Jack A. Adams

6. REPORT DATE	7a. TOTAL NO. OF PAGES	7b. NO. OF REFS
September 1971	18	20

8a. CONTRACT OR GRANT NO. F44620-70-C-0105	9a. ORIGINATOR'S REPORT NUMBER(S)
b. PROJECT NO. 9778	ARL-/1-19/AFOSR-71-6
c. 61102F	9b. OTHER REPORT NO(S) *(Any other numbers that may be assigned this report)*
d. 681313	AFOSR - TR - 72 - 1662

10. DISTRIBUTION STATEMENT

Approved for public release; distribution unlimited

11. SUPPLEMENTARY NOTES	12. SPONSORING MILITARY ACTIVITY
TECH,OTHER	Air Force Office of Scientific Research 1400 Wilson Boulevard (NL) Arlington, Virginia 22209

13. ABSTRACT

The vigor of engineering psychology as an applied discipline in engineering and psychology is dependent upon the robustness of the scientific knowledge that it applies to the design of man-machine systems. As a field, engineering psychology mostly has its practitioners applying knowledge and comparatively few generating new knowledge, with the result that the capability for system innovation is not as strong as it should be. Project Hindsight of the Department of Defense and Project TRACES of the National Science Foundation show that rather long-term basic and applied research is necessary for generating the knowledge that brings impressive innovations in products. The demands of product development have called for too much short-term research and too little long-term research of the kind most effective for producing important innovations in new systems. Several remedial courses of action are considered.

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DD FORM 1473
1 NOV 65

14. KEY WORDS	LINK A		LINK B		LINK C	
	ROLE	WT	ROLE	WT	ROLE	WT
Engineering Psychology						
Basic Research						
Applied Research						
Product Innovation						
Project Hindsight						
Project TRACES						

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RESEARCH AND THE FUTURE OF ENGINEERING
PSYCHOLOGY

Jack A. Adams

University of Illinois

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FOREWORD

This paper was presented on September 4, 1971 as a presidential address
to The Society of Engineering Psychologists, Division 21 of the American Psychological
Association. The production of this report was supported by the Air Force Office of
Scientific Research under contract F44620-70-C-0105.

RESEARCH AND THE FUTURE OF ENGINEERING PSYCHOLOGY

Jack A. Adams

It is customary for presidential addresses to have an onward and upward tone, often giving examples of new research or of the field's contribution to the solution of society's problems. Everyone likes assurances that his scientific field is making progress, and presidents of professional societies hasten to fill this need. For a change, however, I am going to create dissonance and suggest that all is not well with engineering psychology.

In criticizing engineering psychology, I do it in appreciation of the contributions that we have made to the design and use of man-machine systems. Most of our members work for industry, where management is hardheaded, with a steely eye fixed on costs and products, and if our members were not paying their way I am sure that long ago they would have been declared unfit in the struggle for existence in the marketplace. But in acknowledging our accomplishments, I believe that the knowledge on which our discipline is based has been accruing at a dangerously slow rate, and that we are too content with old knowledge. The consequence is a slower rate of accomplishment than we might have had, and pessimistic projections for the future. This, then, is the thesis of my address: Our research efforts have been and are insufficient. The future of engineering psychology is in jeopardy unless we examine realistically the state of our knowledge and ask what we must do to strengthen it.

There are many in human factors who worry about the status of their speciality, struggle for identity with respect to other disciplines, worry about their acceptance by engineers, management, pilots, the military, and so on. Such insecurities are symptomatic of our shaky scientific position. The world that interacts with engineering psychology has its resistance to accepting the new, just like all people and institutions, but it isn't stupid and more often than not it knows a good thing when it sees it, particularly if it means efficiency and profits. That the world embraces us as much as it does speaks for our scientific strength;

that it sometimes has doubts about us should make us ask about the solidity of our scientific platform.

No matter how we cloak our efforts in systems terminology, we can't escape the basic truth that we are trafficking in the same scientific stuff as the rest of psychology -- the prediction of behavior. When we specify the design of a console, the characteristics of a dial, the layout of a panel, or the type of personnel to man a system, we are saying that our specification will give better performance for the system than some other specification. We are making a prediction about behavior, hopefully accurate enough to optimize performance of the system at some later time when the system is completed. In so doing, we strive to fulfill the first premise of engineering psychology -- that man is an integral part of man-machine systems just like the hardware, and man's performance must be optimized, just as hardware components, if the system as a whole is to be optimized.

When we predict behavior in the system design context, we generalize ri the scientific know ledge what we have, and our vigor as a profession comes the strength of our data. A gratifyingly high proportion of engineering psy   s are trained scientists, and their research products constitute the essential scientific information which we apply to system problems. What is distressing is that so many of these scientists seem to be content with a low level of scientific knowledge in so many areas. If it is protested that psychology is young, and a modest scientific capability is the way it is, I can only say that the rest of psychology seems more restless about its ignorance than we are. We have a surprising acceptance of our ignorance; we don't doggedly persist in research on primary topics and pile up the experiments that give the reliable knowledge that is a mature science. I have often wished that engineering psychology would show a fraction of the research enthusiasm that some of the other APA divisions show. For example, in the past ten years, the short-term memory system in humans, as a relatively small aspect of behavior, has had hundreds of experiments performed on it, theories constructed about it, symposia held on it, debates waged about it,

dissertations by the scores done in its behalf, and endless lectures and seminars devoted to it. This isn't a matter of basic versus applied science, where basic science has spirited debate because academics have always enjoyed intellectual nit-picking and applied science has little of it because it is too busy in the practical world to bother. APA divisions fully as applied as we are, like the divisions of clinical and of industrial psychology, have an embarrassingly high level of research activity.

SOME EXAMPLES

Let me give a few examples of our research efforts in engineering psychology that had thoughtful beginnings in World War II or shortly thereafter, that were at times pursued with vigor, are still important, but never have gotten far, and are thin for want of research nourishment.

Consider symbolic instrumentation. A problem for over forty years has been the aircraft attitude indicator. In 1929 James Doolittle proved that a pilot could take off and land an airplane by instruments alone, and in his cockpit was the Sperry horizon which was the prototype for the instrument used today. As you know, this instrument has the moving cursor synchronous with the horizon, not the wings of the aircraft. The direction of movement has been controversial for many years and is still unsolved, although my colleague Stanley Roscoe and his associates at the University of Illinois are currently investigating it. The attitude indicator is symptomatic of our indifference towards research on symbolic instruments in general; our journals have little or no research on the topic, year after year. A recent Air Force technical report by Semple and his associates (Semple et. al., 1971) noted, as I did in 1967 (Adams, 1967), that we haven't yet answered the old question of the relative superiority of linear versus circular scales.

Consider pictorial displays. One of the exciting display ideas after World War II was that pictorial presentations might effectively replace symbolic instruments and give superior performance for two main reasons: (a) they provide instrument integration, combining several data sources into a coherent whole, and (b) they

capitalize on the operators' past perceptual experience with relationships in the real world. Williams and his associates at the University of Illinois (Williams and Roscoe, 1949; Roscoe et. al., 1950) did pioneering work on pictorial displays for aircraft navigation after World War II, and these ideas are now being considered for operational aircraft. This part of the story is satisfactory because it represents an orderly progression from laboratory research to engineering development and use. But the story for the companion effort on contact analog displays is not so pretty. Using the same logic as for the pictorial presentations that were successful in the decision-making associated with aircraft navigation, it was reasoned that a corresponding dynamic display might prove superior to symbolic instruments for vehicular control. A considerable amount of impressive work was carried out by behavioral scientists at Bell Helicopter and Electric Boat Company under the Army-Navy Instrumentation Program (Matheny and Hardt, 1959; Elam, Emery, and Matheny, 1962; Sidorsky and Newton, 1959; Blair and Plath, 1962; Dougherty, Emery, and Curtin, 1964; Abbott and Dougherty, 1964; Sidorsky, 1958; Fox, Hardt, and Matheny, 1959), but their work raised more questions than were answered. Little research on contact-analog is being done today, as far as I know, and the whole complex topic lies without a solution.

Consider training devices. I wouldn't consider the money being spent on flight simulators as staggering if we knew much about their training value, which we don't. We build flight simulators as realistic as possible, which is consistent with the identical elements theory of transfer of Thorndike, but the approach is also a coverup for our ignorance about transfer because in our doubts we have made costly devices as realistic as we can in the hopes of gaining as much transfer as we can. In these affluent times the users have been willing to pay the price, but the result has been an avoidance of the more challenging questions of how the transfer might be accomplished in other ways, or whether all that complexity is really necessary.

Consider workspace. Maybe more than anything else, engineering psychologists in industry probably concern themselves with the layout of panels and workspaces. Empirical work on this topic has been intermittent, although we prescribe endlessly about it.

Consider tracking. Research on tracking has fallen out of fashion in the past fifteen years. One of the reasons is that we were impressed with the rise of automatic control systems and computers and frequently said that man was on his way out as a controller and that increasingly his role would be that of a system manager. The executive role for man is, indeed, increasing. Nevertheless, looking around us, we see as much manual control as ever, from bicycles to space vehicles. Perhaps we neglected to see that the world may be too uncertain to ever fully automate, that automatic controls can be too expensive, or that automatic control systems need man as a redundant subsystem to maintain system reliability when automatic systems fail. Tracking is another topic on which some research was done, and which we prematurely abandoned.

And so it goes. Each of us has his own list of areas which he feels are lagging and which need a research push.

SOME REASONS

Why haven't we been doing more substantive research? There are several reasons:

1. Engineering psychology has been successful. Most of our members are productively engaged in government and in industry, and these are often nonresearch situations. When research opportunities occur, the call is often for short-term engineering research, like the comparing of A and B, or the measuring of performance in a test situation.

2. Too much of our research money comes from agencies with specific engineering interests and deadlines. They tend to give only short-term R & D money that is ill-suited for the development of significant knowledge, as I shall document later. Short-term research has its uses, of course, but the development

of significant and generalizable knowledge is not one of them.

3. We are fooled by human engineering handbooks which imply that we know more than we do. Some of the generalizations in handbooks, as far as I can tell, have no supporting research evidence at all; they are an author's best guess. This is what Ira Abbott (1960), as director of NASA's Advanced Research Programs, once called sophistry, where hypotheses appear as conclusions to serve as the busis for more sophistry.

PROJECTS HINDSIGHT AND TRACES

Rather than discuss our shortcomings further, I would like to take a more positive approach and present results of two recent studies which document the value of research for systems, and which give insights into what we must do. One is on mission-oriented or applied research, and the other is on basic research. What is interesting about these two studies is that they show how basic and applied research interact and culminate in socially desired products, and how long it takes to do it. The successes of the other sciences have lessons for those of us who are interested in increasing the uses of psychology.

The first study that I will discuss is Project Hindsight, and it was conducted by the Department of Defense (Isenson, 1967). The study asked if DOD was getting its money's worth for the billions spent on applied research. Twenty weapons systems were examined, among them being such well-known systems as the Bullpup air-to-surface missile, the Polaris submarine-launched ballistic missile, Minuteman I and II ballistic missiles, the Mark 46 antisubmarine torpedo, the Starlight Scope for passive night vision, and the C-141A strategic transport aircraft. The procedure was to work backwards from an important innovation in the system and ask what R & D events were responsible for it. The unit of analysis was the R & D event, and it is equivalent to an idea from an experiment, a scientific insight, or an invention. It is a research product. These R & D events were traced back to 1945 -- about 20 years, and 710 of them were found.

The outcome was a triumph for applied research. Ninety-one percent of the R & D events could be classified as applied rather than basic research. In 98 percent of the cases the investigator was motivated by his awareness that a problem existed, not curiosity or extending the bounds of knowledge. And, as might be expected for applied research, government and industrial laboratories contributed most of the R & D events, not universities. Table 1 shows where these events came from.

Table 1

Project Hindsight

Sources of R & D Events for Applied Research

Research Agency	Percent
Universities	8
Industrial Laboratories	44
In-House Laboratories of the Department of Defense	48

Figure 1 gives the time distribution of the 710 R & D events for the 20 systems. Sixty-seven percent of the events occurred long before the system was begun. The median time between occurrence of a R & D event and use of the knowledge in a weapons system was nine years. Research which has an impressive innovative effect on systems is not short-term engineering research which is done on the system itself, but is often relatively long-term work that occurs well ahead of immediate need. Furthermore, R & D events were often not specific to a weapon system but were generally useful throughout a number of systems.

The second study I wish to discuss was performed by the Illinois Institute of Technology for the National Science Foundation. The study is called "Technology in Retrospect and Critical Events in Science," which gives the acronym "TRACES" (Loellbach, 1968; 1969). The investigators for TRACES proceeded in much the same way as those for Project Hindsight, but they removed the time restriction and concerned themselves with long-term basic research events as well as short-term applied ones. You will recall that Project Hindsight restricted its view to 20 years, which is not very long in the history of science.
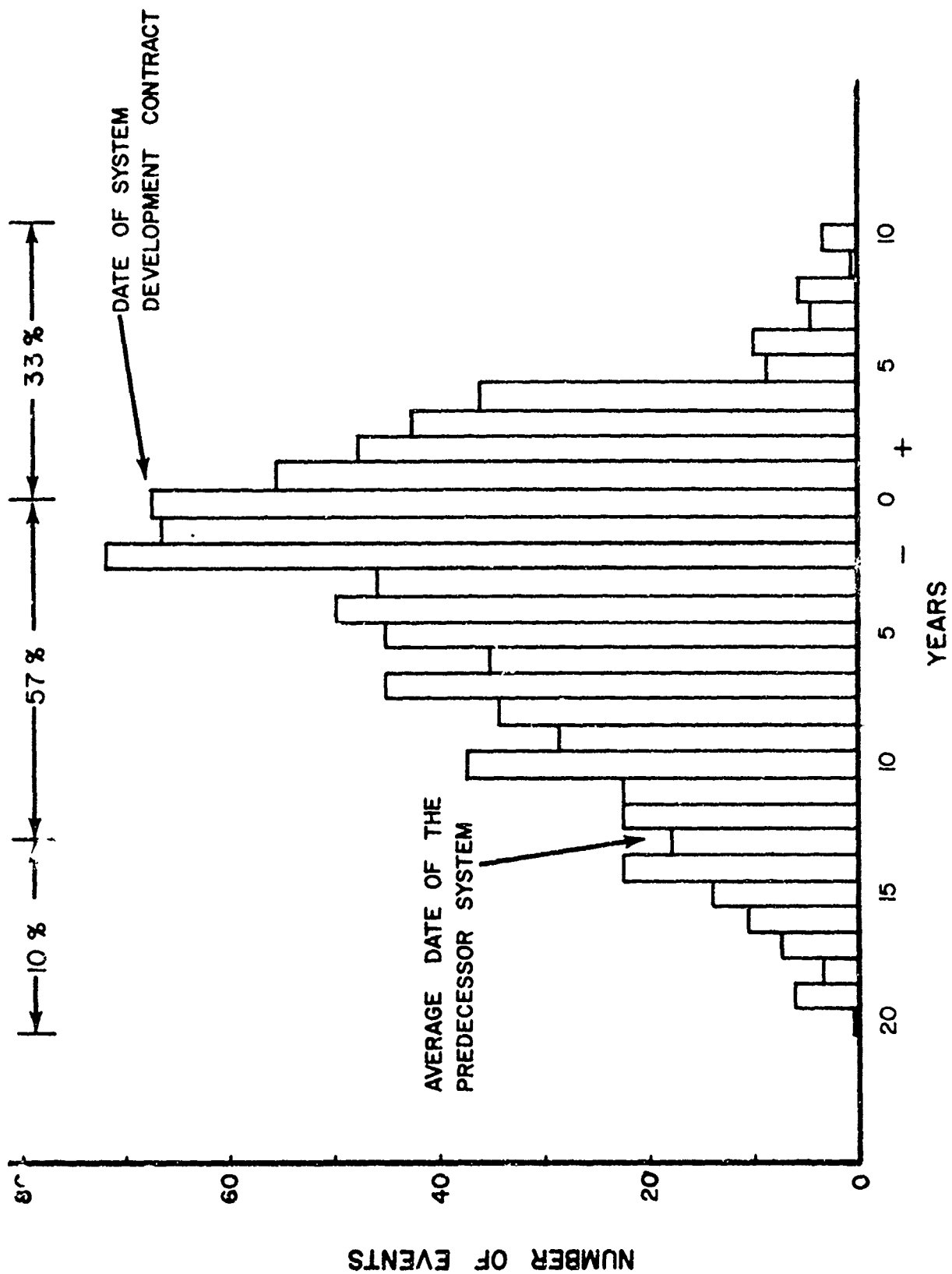
Figure 1. Project Hindsight time distribution of R & D events.

TRACES examined the critical R & D events associated with five socially important products: magnetic ferrites, the video tape recorder, the oral contraceptive pill, the electron microscope, and matrix isolation. (Matrix isolation, I discover, is a new technique which is revolutionizing the chemical processing industry because it is valuable for the study of the mechanisms of chemical reactions.) Three hundred and forty-one R & D events were identified for these five products, and they were classified into three categories: basic research, applied research, and development and application.

When the time limits are removed from the investigation, basic research turns up as a far more potent force than applied research. In contrast to Project Hindsight which found a predominant impact of applied research, TRACES found that 70 percent of the significant events came from basic research, only 20 percent from applied research, and 10 percent from development and application.

Table 2

Project TRACES

Sources of R & D Events

Research Agency	Basic Research	Applied Research	Development and Application
Universities	76	31	7
Industrial Laboratories	14	54	83
Research Institutes & Government Laboratories	10	15	10

Table 2 shows the contributors to the research. Basic research was contributed mostly by universities. As in Project Hindsight, industrial and government laboratories contribute most of the applied research.

Figure 2 relates the R & D events to time. The top two distributions show applied research and, consistent with Project Hindsight, applied R & D events are most prominent in the 20 years prior to product innovation, peaking 20 - 30 years before the innovation, and mainly preceding applied research. Both Projects Hindsight and TRACES make the point that the fundamental discoveries of basic research,

DEVELOPMENT AND APPLICATION

APPLIED RESEARCH

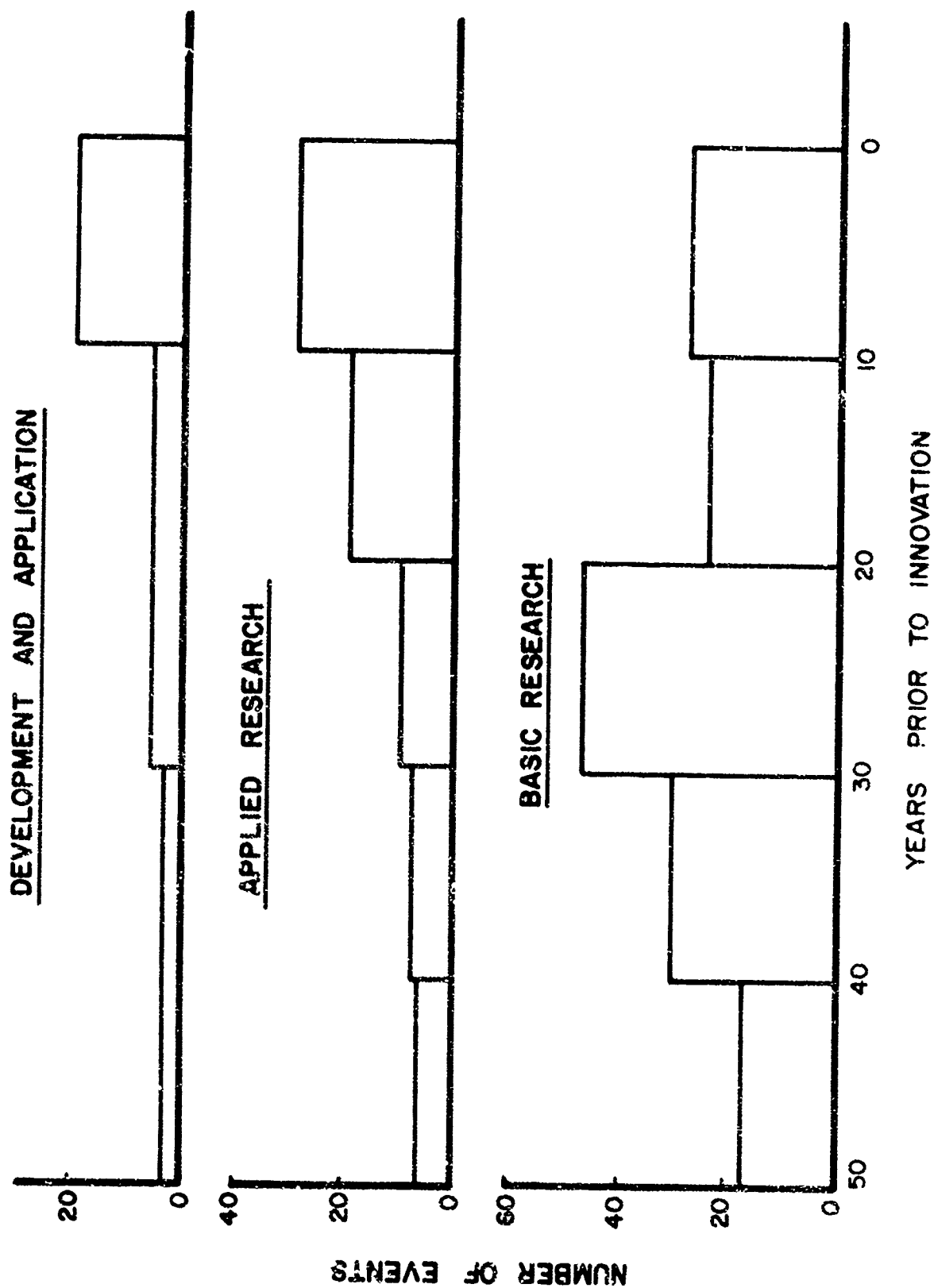BASIC RESEARCH

NUMBER OF EVENTS

YEARS PRIOR TO INNOVATION

Figure 2. Project TRACES time distribution of R & D events.

upon which applied research capitalizes, occur a long time before its practical implications are seen, and the agent of information transmission is higher education where future technologists learn the basic science which they later turn into products. This temporal sequencing of basic and applied research, with education serving as a transitional agent, shows basic and applied research in a harmonious relationship, not the antagonistic relationship in which they have been placed so often.

CONCLUSIONS AND RECOMMENDATIONS

I began with the observation that we are trading on a very modest scientific knowledge, and that our future contributions to man-machine systems are destined to remain modest unless we pursue research with more vigor and expand our technical capabilities. Projects Hindsight and TRACES show that technological advances are intimately tied to research, and the message to me is clear: Unless we accelerate and then maintain a strong level of research productivity, our contributions to systems will fail expectations. Correspondingly, human factors will suffer as a profession.

What can be done about our problem? Should we exhort engineering psychologists to do more research, like a preacher exhorting his congregation to virtue? Instead, let's consider some concrete things that we and others might do.

Applied Research

First, applied research. The research funding agencies for engineering psychology, which is usually DOD, should take a long look at Projects Hindsight and TRACES and ask if the human factors research they are supporting is leading to important innovations in systems. For whatever my reading of the human factors literature for 20 years is worth, I'm convinced that too much funding goes into short-term research that answers local questions for a specific system; the research that is supported typically does not produce influential knowledge that spreads over a number of systems. (On occasion, the Air Force Office of Scientific Research and the Office of Naval Research have made exceptions to this practice.) A main finding of Project Hindsight was that critical R & D events from applied

research typically occurred nine years before the system was begun, and were
events of some generality that spread over a number of systems. Generalizing
to behavioral sciences, DOD should be supporting longer-term applied research
in human factors than it is now doing.

Interest in the long-term support of applied research may come from new
quarters. In 1968 Congress authorized the National Science Foundation to
support applied research. The directive is now being implemented, and it has
implications for us. Long-term applied research in such fields as air traffic
control, traffic safety, and environmental design could involve applied research
of the kind that engineering psychologists do best, and I'm sure that NSF will
increasingly come to look favorably on proposals in these areas. It is also note-
worthy that the National Academy of Engineering, in their report (1970)
advising NSF on priorities in applied research, gives top priority to computers
and their social use, and to psychoelectronics, as they call it, or the application
of electronics to the improvement of human perception, learning, and communication.
These are receptive beds for engineering psychology.

Basic Research

Next, basic research. To what extent should engineering psychologists
engage in basic research? Engineering psychologists are not doing much basic
research because of their strong practical interests, which means that we are
content with accepting the spinoff from basic scientists like our colleagues in
Division 3. There is no doubt that spinoff will occur, because the whole history
of science testifies to the rich interplay between basic science and technology,
and Project TRACES documented that fact nicely. The catch is that it's usually
a serendipitous game, where the scientist goes where his curiosity takes him; and
if the findings prove useful, that's alright, and if they don't, that's alright too. This
is acceptable to me, not because a serendipitous game is a comfortable one to
play, but because the haphazard game works over the long run. Furthermore,
unfettered basic scientists, going where their noses take them, can give

applied investigators a vision they otherwise would not have. I cannot believe
that applied scientists of the 1930's and 1940's who were concerned with the
efficiency of explosives would have ever invented the atomic bomb.

Should we be passive with respect to basic science, patiently waiting
for the spinoff? Twenty to thirty years is the expected time between discovery
of the basic fact and its application. Is there anything we can do to shorten this
time? First, there is the possibility that the relationship between basic and applied
science doesn't necessarily have to be a serendipitous one, that a more calculated
approach to basic science might yield practical results sooner and without con-
straining the scientist's freedom to explore. Should we consider giving some basic
topics a more intensive effort than others, thus hastening the basic knowledge in areas
which we judge most critical? Certainly a selection of topics in terms of eventual
goals goes on more often than we think among basic scientists. William Shockley,
who directed the research effort at Bell Telephone Laboratories on the transistor,
always had in mind the development of a solid state amplifier even though the
program was a basic scientific one without restraints (Nelson, 1959). Improving
the strains of animals and plants for human use has always been an admitted goal
of basic scientists in genetics. Similarly, I suggest that engineering psychologists
should do more basic research and, when they do, I am confident that they will
choose topics that are ripe for exploitation and can be translated into important
technology in less than 30 years.

Another way to shorten the lag between discovery and application is to
improve the transmission of information from basic scientists, who are mostly in
universities, to applied scientists and engineers in government and industry.
The higher education of future technologists is the way it is mostly done today,
but this is a slow process, 20 - 30 years on the average, and we should ask if
there aren't faster ways to turn uncommitted facts into socially useful products.
One way would be for university scientists to communicate to someone besides
students and each other; they should broaden their roles as translators of basic

science and talk more to government and industrial scientists who are close to application. This presumes that university scientists are aware of goals, but in this age of relevance there is a sensitivity to science and its applications, so this may be a smaller problem in the future.

Another approach is for industry to be concerned about cultivating its own translators of basic science. Allen and his associates (Allen, 1970; Allen and Cohen, 1969) at the Massachusetts Institute of Technology, in their study of communication networks in industrial R & D laboratories, have identified a "technological gatekeeper" who functions informally in the role of knowing more about basic scientific findings than anyone else in the organization, and in communicating these findings to his associates. This technological gatekeeper reads more of the professional engineering and scientific journals than the average technologist, and he maintains a wider range of relationships with scientists and technologists outside his own organization. In other words, the technological gatekeeper mediates between his company colleagues and the scientific world outside. A technological gatekeeper is an informal role; he will not be found on the organization charts. Yet, clearly his is an important role for the transmission of basic scientific information to technologists engaged in product development. Allen suggests that this role might be formalized and rewarded.

The Role of Our Professional Organizations

Lastly, professional organizations like Division 21 and the Human Factors Society should play a more active role in the mechanisms of acquiring and utilizing scientific knowledge. We should ask ourselves blunt questions about deficiencies in our knowledge and the kinds of research and research support that are needed to overcome it. The specification of research programs lies with individual scientists, of course, but it would do us good to collectively perform a systems analysis of our knowledge. It is harder to face our weaknesses than to proclaim our strengths, but it may be the frank confrontation necessary to get our knowledge moving.

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